But the most powerful arguments in favour of spontaneous generation are furnished by the study of the monads, the simplest of all organisms, which Haeckel has made the subject of a special monograph. Seven species of these are known, some of which live in fresh water, and some in the sea, all of them consisting of small formless masses of albuminous combinations of carbon, and differing from each other only in their mode of reproduction, development, and nutrition. As these living beings do not present any complication of diverse parts, any division of functions or of organs, as all the phenomena of life with them proceed in a homogeneous manner, and without determinate form, it is very easy to conceive of their spontaneous generation. There is indeed one species which even at the present day appears to be born spontaneously, it is that which has been discovered by Huxley, and which has been described under the name of *Bathybius Haeckelii*. It inhabits the sea at depths of from 12,000 to 24,000 feet; it covers the ground sometimes as a network of strings of *plasma*, and sometimes in little heaps. The nucleus of it seems to be formed by a local condensation of albuminous matter, and the monad becomes a cell. As we have seen above that all animals and plants have their starting-point in a cell, it is allowable to suppose that all species are only monads gradually modified by natural selection.

It will be seen that all the views of Haeckel are founded upon the cellular theory as it has been formulated by Schleiden and Schwann. Haeckel always distinguishes two elements—*cytodes* and cells properly so called, reuniting these two elements under the generic name of *plastides*. Cytodes are parcels of plasma destitute of a nucleus, while cells are plastides endowed with a nucleus; each of these two species being subdivided in its turn into two groups, according as they are or are not enveloped by an exterior membrane. Hence there are four forms of *plastides*: (1) Naked or primitive cytodes, like the present existing monads, the only ones which can proceed directly from spontaneous generation; (2) Cytodes with membranes (*lepocytoda*), proceeding from naked cytodes by a coagulation of the exterior portion of the plasma; (3) Naked or primitive cellules by coagulation of an interior portion of the plasma; and (4), Membranous cells, proceeding either from membranous cytodes by interior condensation of the nucleus, or from naked cells by the external formation of a membrane.

There are two theories, one of which may be adopted. Either the monads at present known are descended by propagation from primitive monads and have preserved the same form for millions of years; or, even at this present time, as was the case in the earlier days of organisation, they owe their existence to repeated acts of spontaneous generation. The latter hypothesis is in no respect less probable than the former.

(*To be continued.*)

SCIENTIFIC SERIALS

Journal de Physique Théorique et Appliquée. Par J-Ch. d'Almeida, Prof. de Physique au Lycée Corneille, Paris. We have before us the numbers of the "Journal de Physique théorique et appliquée" extending over the first year of its existence. The object of the journal, which consists of monthly parts of about forty pages each, is stated in the preface to the first number (January 1872) to be the giving of a new impulse to the study of physical science. This its authors aim at doing "by unfolding the more recent and less known theories, by describing the experiments upon which they rest, by indicating the most easy means of repeating them, and by narrating the progress which physical science makes day by day in France and other countries." By the execution of this project "they hope to interest everyone who is acquainted with the principles of science, to enliven teaching, to excite the spirit of research, and to stimulate discoveries." The journal, which is principally mathematical in its treatment of the subjects, differs considerably from such a journal as the "Philosophical Magazine." The original articles in it are few and unimportant. There are criticisms on and reprints of articles selected from other journals, and bearing on the subjects to which this is devoted; but the distinguishing feature of the journal consists in articles, continued generally through several successive numbers, reproducing and elucidating important theories and investigations, which are thus presented in an easily accessible form. Thus the first number starts off with an article on "Electrostatic measures." We also have a series of articles by M. Terquem on electricity and magnetism, beginning in No. 1 with a short article stating the experiments by which it is demonstrated that free electricity resides only in the surfaces of conductors, and then proceeding in No. 2, and subsequent numbers, to treat of the various units employed in measuring quantities of electricity and magnetism, and the relations which exist between them. The reader will gather the character of such articles as those to which we refer, from the following enumeration of the titles of a few others of those which are more important:—"On the action of a magnet on a magnetised molecule at a distance," "On the analogy between the propagation of heat and the distribution of electricity," "On the propagation of permanent electric currents," "On the employment of the wave theory in optical calculations," "On Electrodynamics and the theory of induction" These articles do not contain original matter, nor for the most part do they contain matter arranged in an original way, but they contain useful and concise expositions of the subjects, reproducing such points as the proof of Ampère's theory, Ohm's laws, the method of comparing electro-magnetic and electro-static units, and the like. Such articles will be principally useful to those who cannot obtain the original investigations, or who desire to avoid the labour of consulting those manuals in which these investigations may be included. The journal seems certainly well calculated to extend a knowledge of and interest in the subjects with which it deals, and thus to assist in achieving the end which it proposes to itself.

JAMES STUART

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 20.—"On the Temperature at which *Bacteria*, *Vibriones*, and their Supposed Germs are killed when immersed in Fluids or exposed to Heat in a Moist State." By H. Charlton Bastian, M.D., F.R.S., Professor of Pathological Anatomy in University College, London. (*Continued from p.* 413.)

The facts just cited concerning the behaviour of thin films of turbid infusions which had been heated to different temperatures gave me the clue as to the proper direction of future work. It would seem that when mounted in the manner described, such thin films of infusion continue capable of supporting and favouring the multiplication of any already existing *Bacteria* and *Vibriones*, although, under such conditions, no new birth of living particles appears to take place even in these fluids. The question then arose as to whether, by subjecting larger quantities of the same infusions to any particular sets of conditions, we could ensure that they also should continue to manifest the same properties. Because if so, it would be almost as easy to determine the death-point of *Bacteria* and *Vibriones* when exposed to heat in these infusions, as it had been to determine it for the saline solutions already mentioned.

It was pointed out by Gruithuisen early in the present century that many infusions, otherwise very productive, ceased to be so when they were poured into a glass vessel whilst boiling, and when this was filled, so that the tightly-fitting stopper touched the fluid. Having myself proved the truth of this assertion for hay-infusion, it seemed likely that, by having recourse to a method of this kind, I should be able to lower the virtues of boiled hay and turnip infusions to the level of those possessed by the boiled saline solution with which I had previously experimented—that is, to reduce them to a state in which, whilst they appear quite unable of themselves to engender *Bacteria* or *Vibriones*, they continue well capable of favouring the rapid multiplication of such organisms.

This was found to be the case, and I have accordingly performed upwards of one hundred experiments with inoculated portions of these two infusions raised to different temperatures. The mode in which the experiments were conducted was as follows:—

Infusions of hay and turnip of slightly different strengths were employed. These infusions having been first loosely strained through muslin, were boiled for about ten or fifteen minutes, and then whilst boiling strained through ordinary Swedish filtering-paper into a glass beaker which had previously been well rinsed with boiling water. A number of glass bottles or tubes were also prepared, which, together with their stoppers or corks, had been boiled in ordinary tap water for a few minutes.* They were taken out full of the boiling fluid; and the stoppers or corks being at once inserted, the vessels and their contents were set aside to cool. When the filtered infusion of hay or turnip had been rapidly cooled down to about 110° F. (by letting the beaker containing it stand in a large basin of cold water), it was inoculated with some of a turbid infusion of hay swarming with active *Bacteria* and *Vibriones*—in the proportion of one drop of the turbid fluid to each fluid ounce of the now clear filtered infusion*. The beaker was then placed upon a sand-bath, and its contained fluid (in which a thermometer was immersed) gradually raised to the required temperature. The fluid was maintained at the same temperature for five minutes by alternately raising the beaker from and replacing it upon the sand-bath. The bottles to be used were then one by one uncorked, emptied, and refilled to the brim with the heated, inoculated fluid.† The corks or stoppers were at once very tightly pressed down so as to leave no air between them and the surface of the fluids. The beaker was then replaced upon the sand-bath and the gas turned on more fully, in order that the experimental fluid might be rapidly raised to a temperature 9° F. (5° C.) higher than it had been before. After five minutes' exposure to this temperature other bottles were filled in the same manner, and so on for the various temperatures the influence of which it was desired to test.

---

* The vessels employed have varied in capacity from two drachms to four ounces; some have been provided with glass stoppers and others with very

Thus prepared, the bottles and tubes have been exposed during the day to a temperature ranging from 65° to 75° F. And generally one had not to wait long in order to ascertain what the results were to be. In some cases, if the contents of the vessels were to become turbid, this was more or less manifest after an interval of forty-eight hours. In other cases, however, the turbidity manifested itself three or more days later, and the reason of this difference will be fully discussed in a subsequent communication.

For the sake of simplicity and brevity, the necessary particulars concerning the 102 experiments have been embodied in the table which will be found below.

The experimental results here tabulated seem naturally divisible into three groups. Thus, when heated only to 131° F., all the infusions became turbid within two days, just as the inoculated saline solutions had done.‡ Heated to 158° F. all the inoculated organic infusions remained clear, as had been the case with the saline solutions in my previous experiments, when heated to 140° F. There remains, therefore, an intermediate heat zone (ranging from a little below 140° to a little below 158° F.), after an exposure to which the inoculated organic infusions are apt to become more slowly turbid, although inoculated saline solutions raised to the same temperatures invariably remain unaltered. The full explanation of these apparent anomalies I propose to make the subject of a future communication to the Royal Society; meanwhile we may quite safely conclude that *Bacteria*, *Vibriones*, and their supposed germs are either actually killed or else completely deprived of their powers of multiplication after a brief exposure to the temperature of 158° F. (70° C.).

This evidence now in our possession as to the limits of "vital resistance" to heat displayed by *Bacteria*, *Vibriones*, and their supposed germs in neutral saline solutions, and in neutral or acid organic infusions, is most pertinent and valuable when considered in relation to that supplied by other sets of experiments bearing upon the all-important problem of the Origin of Life. These latter experiments alone may possibly leave doubt in many minds; but the more thoroughly they are considered in relation to the evidence brought forward in this communication, the more fully, I venture to think, will every lingering doubt as to the proper conclusion to be arrived at be dispelled.

Thus, we now know that boiled turnip- or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with *Bacteria* and *Vibriones* in the course of from two to six days. But, placed under slightly different conditions such as were employed in the inoculation experiments above quoted, although infusions of the same nature do not undergo "spontaneous" putrefactive changes, yet when living *Bacteria* and *Vibriones* are added and not subsequently heated, putrefaction *invariably* takes place and the fluids thus situated rapidly become turbid. There is therefore nothing in the conditions themselves tending to hinder the process of putrefaction, so long as living units are there to initiate it. Our experiments now show that as long as the added *Bacteria*, *Vibriones*, and there supposed germs are subjected to a heat not exceeding 131° F. (55° C.), putrefaction invariably occurs within two days, whilst, on the contrary, whenever they are subjected to a temperature of 158° F. (70° C.) putrefaction does not occur. To what can this difference be due, except to the fact that the previously living organisms which, when living, always excite putrefaction, have been killed by the temperature of 158° F.? It would be of no avail to suppose that the absence of putrefaction in these latter cases is due to the fact that a heat of 158° F., instead of killing the organisms and their germs, merely annuls their powers of reproduction, because in the other series of experiments (with which these have to be compared) where similar fluids are exposed to ordinary or purified air, or are shut off from the influence of air altogether, the most active putrefaction and multiplication of organisms takes place in two, three, or four days, in spite of the much more potent heat of 212° F., to which any pre-existing germs or organisms must have been subjected. The supposition, therefore, that the *Bacteria*, *Vibriiones*, and their germs were not killed in our inoculation experiments at the temperature of 158° F., but were merely deprived of their powers of reproduction, would be no gain to those who desire to stave off the admission that *Bacteria* and *Vibrones* can be proved to arise *de novo* in certain cases. Let us assume this—which is indisputably proved by these inoculation experiments—viz. that an exposure to a temperature of 158° F. (70° C.) for five minutes deprives *Bacteria*, *Vibriones*, and their germs of their usual powers of growth and reproduction—that is that it reduces them to a state of potential, if not necessarily to one of actual death. What end would be served by such a reservation? The impending conclusion would not be staved off by means of it. The explanation of what occurs in the other set of experiments, where the much more potent heat of 212° F. is employed, still would not be possible without having recourse to the supposition of a *de novo* origination of living units, so long as those which may have pre-existed in the flask could be proved to have been reduced to such a state of potential death. It would be preposterous, and contrary to the whole order of nature, to assume that the vastly increased destructive influence of a heat of 212° F. had restored vital properties which a lesser amount (158° F.) of the same influence had completely annulled.

The evidence supplied by these different series of experiments in whichever way it is regarded, as it seems to me, absolutely compels the logical reasoner to conclude that the swarms of living organisms which so often make their appearance in boiled infusions treated in one or other of the various modes already proved to be either destructive or exclusive of pre-existing living things, are the products of a new brood of "living particles," which, in the absence of any co-existing living organisms, must have taken origin in the fluid itself. For this mode of origin of living units, so long spoken of and repudiated as "spontaneous generation," I have proposed the new term Archebiosis.

Inoculation Experiments made with the view of ascertaining the Temperatures at which Bacteria, Vibriones, *and their Supposed Germs are killed in Organic Infusions.*

NEUTRAL HAY INFUSION.

Temp. to which exposed.	Number of Experiments made.	Date of Turbidity, if any.	Results at Expiration of the 8th day.
122° F. (50° C.)	1	24 hours	Turbid.
131° F.	7	48 hours	All turbid.
140° F.	9	1 in 48 hours 6 in 60 hours 1 in 3 days 1 in 8 days	All turbid.
149° F.	4	2 in 5 days 1 in 8 days	Three turbid. One clear.
158° F.	15	… …	All clear.
167° F.	4	… …	All clear.
176° F. (80° C.)	12	… …	All clear.

---

tightly fitting corks; and the latter I find have answered quite as well as the former. On the whole I have found tightly corked 1 oz. phials to be about the most convenient vessels to employ in these inoculation experiments.

* It was found desirable to filter the infusions after they had been boiled, because the boiling generally somewhat impaired their clearness.

† At this stage, of course, *very great care* is needed in order to avoid all chance of accidental contamination either with living organisms or with unheated fragments or particles of organic matter.

‡ In the experiments already referred to.

ACID TURNIP INFUSION.

Temp. to which exposed.	Number of Experiments made.	Date of Turbidity, if any.	Results at Expiration of the 8th day.
122° F.	...	... ...	... ...
131° F.	7	5 in 24 hours 2 in 48 hours	All turbid.
140° F.	12	6 in 40 hours 4 in 3 days 2 in 4 days	All turbid.
149° F.	10	1 in 3 days 3 in 5 days 1 in 7 days 2 in 8 days	Seven turbid. Three clear.
158° F.	17	... ..	All clear.
167° F.	4	... ...	All clear.
176° F.	...	... ...	... ...

March 27.—"On the Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its variation in amount with her Phases." By the Earl of Rosse, D.C.L., F.R.S.

In this paper is given an account of a series of observations made in the Observatory of Birr Castle, in further prosecution of a shorter and less carefully conducted investigation, as regards many details, which forms the subject of two former communications* to the Royal Society. The observations were first corrected for change of the moon's distance from the place of observation, and change of phase during the continuance of each night's work, and thus a curve, whose ordinates represented the scale-readings (corrected) and whose abscissæ represented the corresponding altitudes, was obtained for each night's work. By combining all these, a single curve and table for reducing all the observations to the same zenith-distance was obtained, which proved to be nearly, but not quite, the same as that found by Professor Seidel for the light of the stars. By employing the table thus deduced, and also reducing the heat-determinations obtained on the various nights for change of distance of the sun, a more accurate phase curve was deduced, indicating a more rapid increase of the radiant heat on approaching full moon than was given by the formula previously employed, but still not so much as Prof. Zöllner's gives for the moon's light.

By employing Laplace's formula for the extinction of light in our atmosphere, the heat-effect in terms of the scale-readings was deduced, and an approximation to the height of the atmosphere attempted.

From a series of simultaneous measurements of the moon's heat and light at intervals during the partial eclipse of November 14, 1872, when clouds did not interfere, it was found that the heat and light diminish nearly if not quite proportionally; the minimum for both occurring at or very near the middle of the eclipse, when they were reduced to about half their amounts before and after contact with the penumbra.

PARIS

Academy of Sciences, March 24.—M. de Quatrefages, president, in the chair. M. Faye read a long and exhaustive reply to M. Vicaire's criticisms on his solar theory and attempted revival of Wilson's hypothesis. He answered each objection in detail, and maintained that his position had not been affected by M. Vicaire's arguments.—M. Berthelot read a paper on the constitution of the solutions of the hydracids, and on their inverse reactions. He believes that the increase of heat evolved with increased dilution proves the existence of a number of hydrates, and that the quantity of hydrated acid required to precipitate certain chlorides from their solutions will throw more light on this point.—On certain propyl new derivatives, related to the metallic propyl compounds, by M. A. Cahours.—M. Th. Lestibondois read a note on certain anomalous lianas.—Papers were read on the conditions under which certain periods of the quadratics of a curve of *m* degrees disappear or become infinite, by M. Max Marie.—On a new double-image micrometer, by M. C. Noel.—On the measurement of the chemical effect of solar light, by M. E. Marchand.—On Phylloxera, &c., by M. L. Faucon.—On a new method of determining the position of the nodal surfaces in masses of vibrating gas, by M. D. Gernez.—On the volumetric estimation of carbonic anhydride, by M. Houzeau. The author has devised a method for the application of volumetric analysis to the determination of CO_2 in agricultural chemistry. The process consists in absorbing the gas in soda solution containing zincic oxide, precipitating the carbonate formed with neutral solution of baric chloride, and titrating the free soda left with standard sulphuric acid.—Researches on trichloracetic acid and the trichloracetates, by M. A. Clermont.—On the bursting of the skin of fruits exposed to persistent rain, &c., by M. Joseph Boussingault. The author finds that this is due to the diffusion of water into the fruit. Many kinds of fruit thus absorb large quantities of water, and ultimately burst. The author has also experimented on leaves and stalks. In every case sugar diffused out of the fruit and leaves.—On the snow-line, and its elevation in different parts of the world, by M. Ch. Grad.—On M. Pasteur's process for silk-worm preservation, by M. Guisquet, was an answer to M. Guerin-Méneville, who has denied M. Pasteur's statements.—On the phenomena of hibernation presented by flies exposed to successive changes of heat and cold in Russia, by M. Goubareff. Flies found perfectly torpid became lively at + 33° Réaumur, and became again torpid when the temperature was allowed to fall.—On an optical phenomenon produced by the condensation of dew on grass, by M. J. Leterme.

---

[*] Proceedings of the Royal Society, vol. xvii. p. 436; xix. p. ;

DIARY

THURSDAY, APRIL 3.

ROYAL SOCIETY, at 8.30.—On the Structure of Striped Muscular Fibre: E. A. Schafer.—Note on the Synthesis of Marsh Gas, and the Electric Decomposition of Carbonic Oxide: Sir B. C. Brodie.—On an Air Battery: Dr. Gladstone and A. Tribe.

SOCIETY OF ANTIQUARIES, at 8.30.—Greek Liturgies and Byzantine Architecture: Edwin Freshfield.

CHEMICAL SOCIETY, at 8.—A way of exactly determining the specific gravity of Liquids; Dr. H. Sprengel.—On Cymene from various sources: Dr. C. R. A. Wright—Researches on the action of the Copper-zinc couple on organic bodies, II—On the iodides of Amyl and Methyl: J. H. Gladstone and A. Tribe.—Contributions from the Laboratory of the London Institution, No. XI.—Action of the acid chlorides on Nitrates and Nitrites: Dr. H. G. Armstrong.

LINNEAN SOCIETY, at 8.—On new Indian Fishes: Surgeon-Major F. Day.—On the Fungi of Ceylon: Rev. M. J. Berkeley and C. E. Broome.

ROYAL INSTITUTION, at 3—Coal and its Products: A. V. Harcourt.

FRIDAY, APRIL 4.

ROYAL INSTITUTION, at 9.—Observations on Niagara: Prof. Tyndall.

GEOLOGISTS' ASSOCIATION, at 8—The Diamond Fields of South Africa: G. C. Cooper.—On some Fossils from the Margate Chalk: J. W. Wetherell.

ARCHÆOLOGICAL INSTITUTION, at 4.

SATURDAY, APRIL 5.

ROYAL INSTITUTION, at 3.—Darwin's Philosophy of Language: Prof. Max Müller.

SUNDAY, APRIL 6.

SUNDAY LECTURE SOCIETY, at 4—The Stereoscope, the Pseudoscope, and Binocular Vision: W. B. Carpenter.

MONDAY, APRIL 7.

ENTOMOLOGICAL SOCIETY, at 7.

LONDON INSTITUTION, at 4.—Elementary Botany: Prof. Bentley.

VICTORIA INSTITUTE, at 8.—Force: Prof. Kirk.

WEDNESDAY, APRIL 9.

PHOTOGRAPHIC SOCIETY, at 8.

GEOLOGICAL SOCIETY, at 8—Lakes of the North-eastern Alps, and their bearing on the Glacier-erosion theory: Rev. T. G. Bonney.—Notes on Structure in the Chalk of the Yorkshire Wolds: J. R. Mortimer.

LONDON INSTITUTION, at 7.—Paper and Discussion.

ARCHÆOLOGICAL ASSOCIATION, at 8.

ASTRONOMICAL SOCIETY, at 8.

SOCIETY OF TELEGRAPH ENGINEERS, at 7.30—On a Bell Alarm for Submarine Cables: Wm. F. King—On the Measurement of Battery Resistance: Jas. Graves.—On the Mechanical Tests of Iron Wire: R. S. Culley.

THURSDAY, APRIL 10.

MATHEMATICAL SOCIETY, at 8—On Systems of Porismatic Equations, Algebraical and Trigonometrical; Note on Epicycloids and Hypocycloids; Locus of point of concourse of perpendicular Tangents to a Cardioid; Elliptic motion under acceleration constant in direction: Prof. Wolstenholme.—On the calculation of the Value of the theoretical unit-angle to a great number of decimal places: Mr. J. W. L. Glaisher.

CONTENTS

PAGE
ORIGIN OF CERTAIN INSTINCTS. By CHARLES DARWIN, F.R.S. . . . 417
UNIVERSITY OARS, II. By ARCHIBALD MACLAREN 418
THE EARTH (*With Illustrations.*) 421
LETTERS TO THE EDITOR:—
    Dana on Corals—Prof. A. E. VERRILL 423
    Animal Instincts.—E. W. Cox. Serjeant-at-Law 424
    The Sociability of Cats.—J. JEREMIAH 425
    Manitoba Observatory.—G. T. KINGSTON 425
ST. THOMAS CHARTERHOUSE TEACHERS' SCIENCE CLASSES . . . 425
THE TROGLODYTES OF THE VEZERE (*With Illustrations*), IV. By PAUL BROCA 426
PROF. FLOWER'S HUNTERIAN LECTURES 428
AN ENGINEERING COLLEGE IN JAPAN 430
NOTES 430
THE THEORY OF EVOLUTION IN GERMANY, II. 433
SCIENTIFIC SERIALS 434
SOCIETIES AND ACADEMIES 434
DIARY 436